1. Is science fiction?

In October 1981, I was searching through the papers of the late T. M. Sonneborn at Indiana University. At that time, I was a doctoral student in the history of science at the University of Montreal. I was working on the history of genetics, more precisely, the study of cytoplasmic inheritance, an area that had been virtually ignored by historians of modern biology. Sonneborn was one of the central figures in the development of modern genetics; he was widely respected as a brilliant experimentalist, with the broadest grasp of fundamental biological problems. I had some correspondence with him, and after his death in 1981, his wife, Mrs. Ruth Sonneborn, generously invited me into her home and permitted me to search through his unpublished papers and professional correspondence. It was there that I found a file marked "Moewus" that held together a great body of correspondence which told a remarkable story. The letters told of a major controversy in the origins of what is now molecular biology surrounding the work of the German biologist Franz Moewus.

I had never heard of Moewus. But as I read on, I learned that during the late 1940s and early 1950s, Moewus was hailed by many biologists as one of the outstanding leaders in biological research of this century and one of the principal architects of the revolution in modern biology. Moewus was a pioneer in the development of microbial genetics. When, in the early 1930s, geneticists did not know if microorganisms had genes like higher organisms, Moewus provided some of the first demonstrations that microorganisms did possess genes that were inherited in the classical Mendelian way. During the late 1930s, when geneticists did not know what a gene was or how it worked, Moewus led the way again by providing basic concepts and important methodologies. He also provided the experimental "facts" concerning the biochemical means by which genes affect sexuality in one microorganism.

Yet, during the late 1930s, 1940s, and 1950s, Moewus did not always receive credit for his insightful concepts and pathbreaking methodologies. Instead, he was relentlessly criticized and defamed by many others. There were charges that Moewus's data were "too good to be true," that he had "polished off" his data, and that some of his interpretations were faulty. There were stories about Moewus's refusal to send his cultures to others or his sending dead cultures to those who wanted to repeat and extend his work. During the early 1950s, there were several failed attempts to repeat some of his experiments. Some geneticists interpreted these failures to be clear disconfirmations of Moewus's results and further indication that his reports were unreliable. Others dismissed the disconfirmations of Moewus's work as being faulty and argued that Moewus deserved the highest recognition regardless of the criticism that had been made against his work. Even when the controversy came to a close, leading geneticists believed they still had no direct evidence for invalidating any specific observation, experiment, or idea set forth in Moewus's publications. They had only circumstantial evidence. Although Moewus himself continually denied the charges made against him, he was ultimately judged guilty of perpetrating one of the most ambitious cases of fraud in the history of science. Shortly thereafter, Moewus died of a heart attack on May 30, 1959.

The story, as I first learned of it in Sonneborn's letters, caught my imagination. It conflicted with the little I knew of the history of genetics and of human nature. I, along with a whole generation of scientists and historians, had been brought up believing that George Beadle and Edward Tatum (1941) were the first to show how microorganisms could be used for investigating genic action. I believed they had laid the foundations for the biochemical genetics of microorganisms. In 1958, they were awarded a Nobel Prize, which they shared with Joshua Lederberg. I wanted to know more. I consulted texts on the history of genetics. I soon found that Moewus's work was excluded from all of the many texts celebrating the historical development of modern genetics and the triumph of molecular biology (see, for example, Dunn, 1965; Sturtevant, 1965; Olby, 1974; Allen, 1978a; Judson, 1979). Those that did mention his name (Sturtevant, 1965; Olby, 1974) alluded to him only in passing.

The Moewus story, like the research on cytoplasmic inheritance, represented another case of historians' "neglect." But the telling of the story had to wait until I completed my thesis on the history of cytoplasmic genetics. By that time, I hoped I would know more about the history of genetics. When I finished my doctoral dissertation, in 1984, coming to grips with the historical neglect of Moewus was not that difficult. I came to recognize more and more the biases of scientists and historians which shape how the "past" is constructed. These biases stem not only from "reading into" the literature by superimposing modern ideas on past scientific work; they result also from omitting historical facts that conflict with the historians' preconceived views of the nature of science. In history, the unconscious biases of the writer in selecting and interpreting data is not called fraud. We have another word for it. These kinds of accounts are quaintly labeled "Whiggish."

The claim that Moewus had polished off his data did not bother me. As I will explain momentarily, I suspect that all scientists do this. Moreover, I knew that Mendel's results were also "too good to be true," but no one denied his contributions and excluded him from history. Indeed, he is universally hailed as the "founding father" of the entire science of genetics. What I found difficult to digest was that Moewus had deliberately fabricated his results in a wholesale way. Moewus's biochemical genetic results seemed to be too elaborate and consistent to be fabricated in a wholesale way. For me to believe *that* would be similar to the reader's believing that I am fabricating the entire story that will unfold before you. As you will soon recognize, to do this would be very clever indeed. Moreover, the breadth of Moewus's accomplishments – his ideas and methodology – seemed to be too sophisticated to have originated with someone who would completely fabricate his data. And why would anyone who was obviously as intelligent as Moewus go to the trouble of faking his experiments when he could actually do them?

I turned to accounts of other controversies involving fraud charges to help unravel these questions. I read Koestler's (1971) account of Paul Kammerer and the midwife toad, and Weiner's (1955) account of the Piltdown forgery. But it seemed that each of these cases represented a hoax – a not-so-funny joke. Moreover, it was perfectly clear that someone had put the India ink on the toads to prove or disprove the inheritance of acquired characteristics. And someone deliberately had placed the skull of a human with the jaw of a monkey to be later discovered as the "missing link." The crime in these two cases is clear. The remaining mystery is a "Who dunnit?" The Moewus case was different. The suspected fraud was much more elaborate, and there was no clear-cut evidence suggesting that he had committed a crime. This problem warranted investigation. It was clear to me that I needed to know more about how scientists assess and evaluate knowledge claims.

In the meantime, I needed a job. And I soon found one. After working for nine months as visiting assistant professor in the History Department of the University of Arizona at Tucson, I flew back to Montreal to defend my doctoral dissertation. The next morning I was on a plane bound for Australia, where I was to take up a position as lecturer in the History and Philosophy of Science Department of Melbourne University. Here, I found excellent teaching and research conditions.

I immediately began to work on the Moewus story again, this time in earnest. My work was interrupted only by having to polish up for publication my thesis on cytoplasmic inheritance (Sapp, 1986, 1987a). As I began to collect the published literature concerning Moewus, I came to realize that the story was much more complex than I had imagined. There seemed to be an inside story that could not be fully revealed by studying the published scientific literature alone. It was clear that if this story was to be told in sufficient detail, I needed help from those scientists who had participated in the controversy. I immediately began to write them, asking for literature and advice, and inquiring whether I could arrange an interview.

My inquiries were met with overwhelming help and encouragement. Virtually all of the participants I contacted cooperated in any way they could. All of them wanted this story to be told as thoroughly as possible. Many sent letters of correspondence they had with or about Moewus, as well as names and addresses of others I should be sure to contact. All of those who were asked agreed to be interviewed. Others, including Bernard Davis, D. L. Nanney, and R. W. Kaplan, wrote me lengthy letters. Moewus's scientific career spanned three continents – Europe, Australia, and North America. In order to retrace his steps, I needed travel money. The Australian Research Grant Scheme generously provided funds for this project. My German is poor, so I hired Bernd Bartl, a German doctoral student, to help with translation.

This book could not have been written without the help of Mrs. Ruth Sonneborn, and many others. Upon arriving in Australia, I interviewed the leading Australian biochemist, Arthur Birch. Birch and Moewus had written important papers together in the early 1950s. Soon after, through the help of David Nanney, I learned that the German protozoologist Karl Grell often spent the winter working at the Australian Institute for Marine Sciences in Townsville, off the Great Barrier Reef. I happily flew to Townsville to talk with him about the conditions in Germany, where Moewus had done most of his work. My next travel leave took me to the East and West Coasts of the United States, where I met John A. Moore and his wife, Betty Moore, at the University of California, Riverside. John Moore was instrumental in bringing Moewus to the United States in the 1950s. No one was more helpful and encouraging than Ralph Lewin, a leading algologist at the Scripps Institution of Oceanography, La Jolla. In the early 1950s, Lewin had been working along lines similar to Moewus's; he became, in effect, one of his chief adversaries. Lewin had followed the Moewus stories closely, and he had long hoped that someone would write a detailed account.

In New York City, I was able to talk with Joshua Lederberg, president of Rockefeller University. Lederberg told me about the political gossip about Moewus that he had heard as a young pioneer in the development of microbial genetics. On the outskirts of Columbia University, I spent an evening with Mrs. Elizabeth Ryan, wife of the late Francis Ryan. Francis Ryan was a leading bacterial geneticist at Columbia University who had invited Moewus into his laboratory in an attempt to repeat some of his work in order to help bring the controversy to a close. James D. Watson, director of the Cold Spring Harbor Laboratory on Long Island, generously provided lodging for me and told me how stimulating he had found Moewus's work on the eve of his own discovery, with Francis Crick, of the structure of DNA. Ruth Sager, director of the Division of Cancer Genetics at Harvard University, told me of her meetings with Moewus at the Marine Biological Laboratory, Woods Hole, Cape Cod.

While on the East Coast of the United States, I took the opportunity of searching through the archives of the Rockefeller Foundation in Tarrytown, New York, for information on Moewus and the institutions in which he worked. The Natural Science Division of the Rockefeller Foundation, headed by Warren Weaver, played a leading role in fostering the development of molecular biology. Rockefeller officials kept detailed diaries of interviews with scientists and their visits to scientific institutions. I learned of the attitude of Rockefeller officials toward providing support for Moewus and others who attempted to repeat his work and bring the controversy to a close. I also learned about the political basis of their funding policies during the decade following World War II and gained some insight into the thinking of the directors and the internal workings of the two main institutions involved in Moewus's work.

The following year, I was able to travel to Heidelberg to spend several hours talking with Franz Moewus's widow, Mrs. Liselotte Kobb. Mrs. Kobb was a respected scientist in her own right and had frequently assisted her husband in the laboratory and in writing his scientific papers. She is completely convinced that there was no intended deception in her husband's reported observations. Mrs. Kobb generously shared with me some of her happiest and saddest memories of her life with her husband. She allowed this story to be written with the hope that the lessons we all might learn from it will not be lost.

When my journey was ended, I returned with a story that is, above all, one about the human side of science – a story about scientific truth, authority, war, racism, sexism, national pride, and individual dignity. This was hardly a simple story of a psychopathic scientist who managed to fool a naive community of researchers. Moreover, one point was certainly clear: When the controversy ended in the mid-1950s, much was unsettled in the minds of those scientists who dismissed Moewus as the perpetrator of a fraud. Moewus's judges based their opinions on a diverse and scattered body of circumstantial evidence. No one was certain about how much data Moewus "falsified," what could be retained as "true," and what had to be discarded as "false." Nor was anyone certain about Moewus's motives for perpetrating a "fraud." But, if it was fraud, as many participants believe, then it was indeed one of the most ambitious of its kind in the history of science. On the other hand, it seemed to be entirely possible that Moewus's "crime" may have been constructed by the scientific community itself. Again, I turned to the secondary literature on "fraud" for some insights to help unravel this mystery.

The normal and the pathological

In recent years, there has been a spate of disclosures of fraud in science. Indeed, with so many public disclosures of fraud occurring constantly, it is proving difficult to keep up with them. Fraud has been uncovered in all areas of scientific activity and all levels of the scientific community, from the "hired hand," to industrial scientists, to those in the "mainstream" of the academic scientific community (Bridgstock, 1982). In each of these areas, investigators have explored the social conditions that might encourage fraud and the motives of the researchers. Generally, they have traced the occurrence of fraud to a consideration of three factors: the rewards, the perceived risks of getting caught, and the honesty and integrity of the individual. Hired hands are often willing to take shortcuts to obtain the specific answers sought by supervisors. They are also generally interested in making money, and usually there is no career to be threatened – at most, a temporary job (Roth, 1966). In industrial science, fraud frequently occurs when scientific tests are performed solely to satisfy a government bureaucracy of a safety requirement, or when research is done for publicity purposes. The case of thalidomide clearly illustrates what can happen when scientists become totally committed to a company that has pinned all its hopes on a product currently undergoing safety tests (Knightly et al., 1979).

Many cases of fraud have been recently disclosed also in the mainstream of scientific research. They have been widely publicized by the science journalists Broad and Wade (1982). Two of the best-known recent examples are the so-called painted-mice affair (Hixson, 1976) and the case surrounding Cyril Burt (Kamin, 1974; Hearnshaw, 1979). William Summerlin, a scientist at the prestigious Sloan Kettering Institute, painted the skin of two mice to demonstrate the success of his newly developed techniques of skin transplantation. Summerlin confessed to his fraudulent act. Whereas the research administrators claimed that Summerlin's behavior was the result of a deranged mind (temporary insanity), Summerlin claimed that he was put under a great deal of pressure from his superiors to produce positive results (Broad and Wade, 1982: 153–157).

Sir Cyril Burt died before his work came under serious scrutiny. Burt, one of the pioneers of applied psychology in England, invented in a wholesale way his I.Q. test data and even the very existence of his coworkers in order to support his theory that intelligence is determined by heredity. His official biographer, Hearnshaw (1979), believes that Burt's research reports from 1943 onward have to be regarded with suspicion. But his fraud went undetected for 31 years (Kamin, 1974). Two circumstances have been proposed to explain how Burt was able to pass off his elitist hereditarian opinions as fact without severe scrutiny. First, Burt held such a powerful position in the psychological establishment that he became immune to scrutiny. He was the editor of the *British Journal of Statistical Psychology* and used his position to publish numerous articles under pseudonyms. Because of Burt's prestige in the field, those who were critical of his work were afraid to make their views public. Second, Burt's data fit the dominant views of his times. People believed what they wanted to believe. Kamin, the first to attack publicly the legitimacy of Burt's data, was a socialist who adopted an environmental view of human intelligence. Burt's fraud is particularly disturbing since his data had a serious effect on education policy in Britain and the United States.

These and some other less publicized cases have led several writers to believe that the known cases of fraud may represent only the tip of the iceberg (see Rensberger, 1977; Gould, 1978; Weinstein, 1979; Broad and Wade, 1982; Martin Bridgstock, 1982; Chubin, 1985). In recent years, members of almost every major scientific institution have been forced to acknowledge that fraud occurs and have come to realize they must deal with it in some sensible way. In the relations between science and society, professional scientists are concerned about maintaining the reputation of scientists as purveyors of truth. In their relations with funding agencies, members of scientific institutions need to protect their reputations against charges of misusing funds. In their relations with each other, scientists want to rely on the trustworthiness of their colleagues' data. Lastly, scientists want to protect their intellectual property rights and guard against plagiarism. Formal guidelines have been proposed by scientific organizations to prevent fraud and to protect the innocent from irresponsible charges, while simultaneously encouraging individuals with certain knowledge of wrongdoing to make it known in appropriate ways.

Any attempt to understand fraud in science necessarily reflects one's view of how science properly functions, just as the pathological reflects the normal. Virtually all discussion of misconduct in science begins with the basic "rules" of science – the norms of science first put forward by Robert Merton, a pioneer in the sociology of science. In 1942, Merton briefly summarized a series of institutional imperatives for science. Along with technical or cognitive norms, such as requirements of logical consistency and empirical confirmability, Merton's rules of science consisted essentially of four moral normative requirements which he believed comprised the ethos of modern science. The moral norms that Merton held were necessary for the extension of certified scientific knowledge may be listed as follows (see Merton, 1973):

1. *Universalism:* This norm requires that knowledge claims are evaluated in terms of cognitive criteria; not in terms of personal attributes of their authors. In other words, the social standing of the scientist making a claim (i.e., whether he or she is an assistant researcher or Nobel Prize winner) should not significantly affect the judgments of others toward the knowledge being assessed.
2. *Communism:* The findings of scientists are a product of social collaboration and thus belong to the scientific community as a whole. Scientists do not *own* their work; intellectual property is limited to peer recognition. All information is made public, and secrecy is avoided.
3. *Organized skepticism:* Knowledge claims must be subjected to "detached scrutiny of beliefs in terms of empirical and logical criteria."
4. *Disinterestedness:* A "distinctive pattern of institutional control of a wide range of individual motives characterizes the behavior of scientists" such that it is "to the interest of scientists to conform" by engaging in disinterested activity directed toward the extension of scientific knowledge. Of all Merton's norms,

sociologists have found disinterestedness to be the most difficult to understand. For our purposes it is sufficient to know that most interpret it as a motivational requirement: that scientists are engaged in the pursuit of truth, not prestige or financial gain (see Weinstein, 1979). All have understood fraud in science in terms of a violation of the norm of disinterestedness.

Zuckerman (1977), Weinstein (1979), Bridgstock (1982), Broad and Wade (1982), and others have all shown that studies of fraud and "deviant behavior" are very useful for studying the system of social control in science. If the norms defined by Merton were operative, then certified knowledge would be increased and fraudulent assertions would not be made. Fraud can therefore be used as a probe to investigate how well the so-called "self-regulating structure" of science is functioning. It should be stressed that Merton himself set up the framework for these studies when he argued that the absence of fraud in science was due, not to the personal virtues of scientists, but instead to institutionalized mechanisms of social control. In fact, Merton (1942) made "the virtual absence of fraud in the annals of science" a hallmark of the uniqueness of scientific activity and a necessary corollary of his belief in the "verifiability of results," in "the exact scrutiny of fellow experts" (organized skepticism), and in "rigorous policing to a degree perhaps unparalleled in any other field of activity" (Merton, 1973: 276).

This statement of Merton's about the absence of fraud in science was wrong, even at the time he made it. In fact, in the early nineteenth century, Charles Babbage, the famous English mathematician and inventor of the first modern calculating machine, believed that fraud was prevalent enough in science to classify various kinds. In his well-known diatribe against the elitist nature of the Royal Society, *Reflections on the Decline of Science in England*, Babbage (1830: 174–183) listed various kinds of "frauds of observers":

Hoaxing: The scientist's "deceit is intended to last for a time, and then be discovered, to the ridicule of those who have credited it." The affairs of the Piltdown man and Kammerer's toad may be placed in this category.

Forgery: "The forger is one who, wishing to acquire a reputation in science, records observations which he never made." William Summerlin was found guilty of this practice; Sir Cyril Burt also forged his data on the inheritance of intelligence. However, it is not clear at all, in these two cases, whether the forgers wished only "to acquire a reputation" and were not, in fact, committed to the "truth."

Trimming: The data are manipulated so as to make them look better: "Trimming consists in clipping off little bits here and there from those observations which differ most in excess from the mean, and in sticking them on to those which are too small; a species of 'equitable adjustment,' as a radical would term it, which cannot be admitted in science." In modern terminology this is referred to as "massaging data" or "fudging."

Cooking: According to Babbage, cooking means choosing only those data that fit the researcher's hypothesis, and discarding those that do not; telling half-truths.

In direct conflict with Mertonian views, recent investigations of fraud in science have been devoted largely to showing that adequate policing in science is impossible and that fraud is likely to be endemic in modern science. Pressure to cheat has been traced to the reward system of contemporary science, with its emphasis on the quantity, as opposed to the quality, of publications. The quest for individual recognition and prestige has led scientists to violate the professed norms of science. But this is only half the story. The nature of big science – the quest for big money to finance huge laboratory factories – also seems to be playing a role in eroding the foundations upon which sound and useful knowledge rests. It appears that the success of biotechnologies, such as those associated with recombinant DNA, have set a precedent for science funding policies. They have set unreasonable expectations on other domains of scientific inquiry that are not yet ripe for exploitation. The scientific establishment is beginning to realize how the lure of big money leads them to abandon the ideals of the profession. Investigations of several cases of fraud that have recently appeared in big research laboratories suggest that they may often result from pressure to publish successful results in order to meet the demands set by large funding agencies that want immediately applicable results (see Broad and Wade, 1982).

In principle, replication of experiments, the "Supreme Court" of the scientific system (Collins, 1985), should be a powerful deterrent to fraud. However, in contrast to the common image of science which portrays replication as standard practice, many science analysts claim that replication of another's findings is seldom done in practice since there is little incentive for doing it. Recognition in science is accorded for originality; reward for repeating another's results is granted only in extraordinary circumstances.

One can easily understand how science writers could find a major place for fraud, once barriers to the enforcement of Mertonian norms were detected. These criticisms of Merton's theories generally have been a major source of anxiety and have contributed to the belief that fraud is likely to be endemic in modern institutionalized science. Yet, estimates of the prevalence of fraud in science vary from author to author